Out of the path : in quest of a medium-run growth model

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Introduction

Growth economics and the debates surrounding its development spread throughout the international communities of economists with a 1939 contribution made by Roy. F. Harrod and his "Essay on Dynamic Theory". The number of scientific publications concerning the issue rose significantly from 1956-1957 with the publication of Solow's famous articles "A Contribution to the Theory of Economic Growth" (1956) and "Technical Change and the Aggregate Production Function" (1957) (see table 1 below, from Boianovsky and Hoover, 2009).

Table 1 Attention Paid to Growth by Economists

Period	Articles with "growth" in title (as percentage of all articles)	Change from previous period (percent)
1936–55	0.95	
1956–70	4.64	388
1971–85	2.71	–42
1986–2006	2.67	–2

Data derived from JSTOR journal archive (30 October 2006) based on the *American Economic Review*, the *Journal of Political Economy*, the *Review of Economic Studies*, the *Review of Economics and Statistics*, the *Economic Journal*, and *Econometrica*.

Several communities of researchers took interest in the matter : amongst others, we had the Massachusetts Institute of Technology (MIT), the Universities of Harvard, Stanford – in the United States – and Cambridge, Oxford, the London School of Economics – in the United Kingdom. There was also the Australian economists community : the same year as Solow, Trevor Swan published "Economic Growth and Capital Accumulation", a paper in which the model he presented would be assimilated to Solow's.

In this paper, I shall focus on the communities of the MIT and Cambridge and the interactions they had during the 1956-1968 period concerning the growth issues. I will try to define those relationships, the nature of the questions they addressed – focusing on growth – and see how each community of understanding (Stanley Fish, 1980) dealt with those issues.

When he released his contribution, Robert Solow had been part of MIT since 1949. Based in Cambridge (USA), the Economics Department really took off with the arrival of Paul Samuelson in 1940. In the 1950's, Robert Solow and Paul Samuelson turned the department into a community of researchers, by developing specific graduate and undergraduate courses. (Cherrier,2011) Thanks to their educational vision, they spread a new way of doing economics, recruiting the

best students who would become the best theorists, and that in old and new areas of economics such as growth economics.

On the old continent, economists from Cambridge, UK, were also important contributors to the growth debates. Joan Robinson, Nicolas Kaldor, Luigi Pasinetti took interest in the matter, proposing their own models in opposition of the American "neo-classical" view. Young Amartya Sen at Cambridge since 1953 and young Frank Hahn – he joined Cambridge university as a lecturer in 1960 - had more in-between positions.

The two Cambridges debated a lot around many economic questions. The "Controversies" that characterised their relationship are often presented regarding the question of the nature of the capital but they were about another area of economics : growth. Beyond the simple opposition of two types of models (neo-Keynesian vs. neo-Classical), the problems identified in the growth models developed in the second part of the 1950's run deep in the roots of the economic growth theory. Solow (1956) et Kaldor (1957) both claimed that they were trying to solve the inherent instability of the Harrod-Domar model. By doing so, they developed long run models of growth, very stable but without an independent investment function – savings being at all times equal to full-employment investment. The models presented are always equilibrium models, making the connection with the short run – and Keynesian – model complicated, if not impossible. In a 1960 article, Frank Hahn emphasised the fact that *"[...] no analysis of the behavior of the system when out of equilibrium has been proposed"*, and he tried in his article to study the stability of full-employment growth equilibrium. In 1964, Hahn and Matthews, in their survey of economic growth theory (then by Sen, 1970), pointed out the absence of entrepreneurs expectations in Solow's 1956 model. This absence revealed a limit in growth analysis towards Harrod's original work : Harrod linked instability to failed entrepreneurs expectations, giving an endogenous origin to economic cycles.

Does that mean that any short term related issues were systematically dismissed from growth economics ? Absolutely not. Since the mid-1950's Solow himself showed that he was concerned by the unification of long term – Classical – framework and short-run – Keynesian – framework which would reappear in his later work on what he would call "medium-run" equilibrium (Assous, 2013). But the question is overshadowed by more than one difficulties, the first would be of what was the true purpose of Harrod and Domar's contributions and the way they were interpreted in the 1950's by both communities. It is important to remember at this stage of our argument that the models provided by Harrod were often considered a bit confusing.

Even if it can be stated that those economists simply misread and misunderstood Harrod by trying to build a long-run model of economic growth (Halsmayer and Hoover, 2013), I will support the fact that each economists read Harrod's work from their community of understanding perspective. They interpreted Harrod with the intellectual tools and environment they had access to, challenged by their own members or outsiders concerning the out-of-equilibrium models. The members of MIT and Cambridge interacted with each other, criticising, influencing each other's work. From those interactions resulted major questions on the growth models. Expectations, distribution, investment : they all became at one point or the other limits to the development of those models. Those major issues lead some economists – Hahn, Sen, Stiglitz, Solow himself – to try make a synthesis. As I will maintain, in the last part of this paper, that quest for a synthesis was in fact part of a process to make the possibility of a medium run growth equilibrium emerge.

Cambridge-Cambridge : NOT on the Capital controversies

The Cambridge capital controversy is probably one of the most renowned episode in modern history of the economic thought. Economists from both sides of the Atlantic struggled about concepts that constituted the core of economic theory, such as the capital aggregation problem and the reswitching problem for almost 20 years. Beyond the controversy on the nature of capital launched by Joan Robinson in 1953, the debates heavily influenced the field of growth economics. In this first part, I will present each community and their main contributors in growth theory. Once the characters are introduced, I will expose their mutual growth models. Then, through the concept of interpretative community (Fish, 1980), I will attempt to compare the process that led each community to develop such different models, process that was, as a matter of fact, heavily nourished by regular interactions between the two groups of economists.

1 Economics at MIT : building an intellectual community (1956-1968)

1.1 A (would be brief) history of the Economics Department

The Massachusetts Institute of Technology is over 150 years old. Its Economics Department is relatively young in comparison : it is only in the 1930's that a "department of social sciences" was created (Cherrier, 2011). But in a matter of decades, this department became the place that would form the elite of the economic academic circles. In the 1950's and until the mid-1960's, it recruited the best students and it was at the center of strategical economic decisions through the participation of its people in the Council of Economic Advisers (CEA). The number of Nobel Prizes awarded to MIT people is quite impressive and so are the notable Alumni of the department that occupied key positions in American and international administrations.

Yet, the literature that would describe us the MIT "microcosm" does not seem available. Few are the historians that tried to describe the way things worked in the MIT. Only Mirowsky (2006), Mehrling (2005, 192-5) and Cherrier (2011) seem to have touched the subject of the dynamic that made such a community of economists work. This lack of description can be the result of the several areas in which the Economics Department of MIT excelled and intervened, making difficult to put all its members under a unique designation.

Nevertheless, there is definitely a "MIT-style" that can be traced back to the arrival of Paul Samuelson in 1940. It really took off when Samuelson was joined by Robert Solow at the end of the 1940's. They both shaped the department according to their vision of a new way of doing economics. New tools, new applications, new curriculum : economics at MIT were different. It is a whole community project, with its own internal logic, that can be seen here.

Through their educational project, Solow and Samuelson were building a group of economists that would be self-aware of their own distinctiveness.

Forming Elite Teachers

Solow, more than Samuelson, was especially involved in the education provided at MIT. He was one of the main architects of the professionalization of the economists. The excellence of the curriculum, the teachers and the students enabled the diploma delivered by the MIT to be a sign of credibility (Fourcade, 2009). The MIT economists would be recruited throughout the academic, institutional and corporate sectors. In the 1950's, the Economics department counted also Charles Kindleberg and Evsey Domar (1958) as main figures. Domar would teach comparative economic system and soviet economics, Kindleberg would develop finance. What made MIT so special regarding economics ? Probably, the unusual student oriented teaching department :

"We view ourselves as net exporters of finished economists, [...]"
Solow to Domar (1956).

In 1948, Samuelson published *Economics : An Introductory Analysis*. This textbook had a huge success and spread throughout universities. Undergraduate students were moulded by Samuelson's vision of economics and he himself became notorious. Samuelson carefully updated the next editions of his textbook so that it would remain a "must-read" to economic students (Skousen, 1997). Undergraduate courses took place based on that textbook : it was actually quite rare for faculties to provide undergraduate courses in economics. But the main goal was to form elite teachers in the graduate school of the Economics Department. Each year, around 20 students were recruited. Peter Diamond was one of those students, he completed his PhD in 1961 and would become one of the most important growth theorists of his generation. He had started his years at MIT in the mathematics department before switching to economics (Moscarini and

Wright, 2007). Robert Solow and Charles Kindleberg supervised many students during their PhD dissertations such as Peter Diamond, Alain Enthoven or Robert Mundell. Teaching was at the center of the system put together by Solow and Samuelson to produce high quality economic researchers, that who would themselves train new elite economists. The system seemed to work and the MIT started to lure cutting edge economists from other universities that would continue to develop the graduate and undergraduate curriculum between 1955 and 1965. Robert Solow, Paul Samuelson with the help of Evsey Domar and Charles Kindleberd had managed to build a strong and solid community. Karl Shell joined MIT in 1964 and recalls the atmosphere of the Economics Department (Spear and Wright, 2001) :

"The atmosphere was intense. The MIT economics faculty had lunch at a big table, at which Paul [Samuelson] held court. I was dazzled by Paul—as I expected to be—but I was also impressed by the breadth of general competence in the MIT department. Bob [Solow] was on leave during my first year. When we finally met, I was equally dazzled by Bob—a very deep guy, who tries to make us believe that it all came easily. Bob was splendid at getting the most out of others, always making MIT a happy place. I learned so much from Bob, Paul, Franco [Modigliani], and—of course—my suitemate, coauthor, and friend, Frank Fisher."

Teaching appears like a central element in the dynamic of the MIT of the 1955-1965 period. Could that explain the emulation that took place at the Economic Department at that time ? It could be interesting to find out.

Shell was not the only economist to become affiliate with MIT, there were also : Franklin Fisher, Franco Modigliani, Robert Eckaus, Edwin Kuh, Albert Ando, Stephen Marglin, Joseph Stiglitz, among others.

Being part of the public debates

Another feature that is notable about MIT is the way its members were prominent participants of the public debates. Through their work for the RAND corporation and the several American administrations, MIT-economists spread their ways of thinking and making economics at the political and national level. Many of the MIT-Alumni occupied key positions in the American administrations : Department of the Treasury, Committee for economic development, Attorney General's committee, Bureau of the Budget. MIT's influence and involvement with policy matters became more apparent in 1960, during the presidential election. Samuelson openly supported John F. Kennedy, becoming one of his campaign advisors. In 1960, Samuelson and Solow's estimation of the Phillips Curve allowed them to establish a stable and exploitable structural relationship between unemployment and inflation. This became an argument in favour of the use of inflation against unemployment during the 1960's campaign against Richard Nixon.

In 1961, Robert Solow became part of the Counsel of Economic Advisors where he worked with James Tobin, Walter Heller and Kermit Gordon. Samuelson and Solow, and all economists surrounding them, promoted a new way to apprehend the job of economists : they were experts, who could help to guide the American economic decisions, thanks to their insight of the economic science. The research resulting from the study of those public issues would also come to nourish the curriculum of the MIT-students, making them ready to deal with real life public issues. *The "MIT style"*

As I already emphasised at the beginning, the MIT Economics Department is not easily defined as a unique entity. As a matter of fact, it is complicated to talk about a "school of thought" because the area of theories addressed by its economists are very wide. It might be defined through the tools developed to make economics a theoretical discipline as much as an applied one. Paul Krugman (1995) recalls *"the MIT style : small models applied to real problems, blending real-world observation and a little mathematics to cut through to the core of an issue."*

But the "MIT-style" might also be defined through the community that takes shape when you try to take a closer look at MIT history. In 1995, Krugman, recalling his years in MIT : *"Finally, at MIT I got my first sense of the wider role an economist can play in the world"*. Solow and Samuelson shaped a community that interconnected researchers, teaching and policy worlds in intricate ways.
That special style met real success in the early 1960's and the MIT managed to get top students to enrol in its economics program. In his article about Robert Solow in the New Palgrave Dictionary of Economics, Alan S. Blinder (2008) underlines the fact that *"he [Solow] was also the dissertation adviser of choice for scores of MIT's most promising graduate students over a period of time spanning 45 years. The list of Solow dissertation students, particularly in the 1960s, reads like an all-star team. In the two years 1966 and 1967 alone (based on completion dates), he supervised the Ph.D. dissertations of (in alphabetical order) George Akerlof, Robert Gordon, Robert Hall, William Nordhaus, Eytan Sheshinski, Joseph Stiglitz, and Martin Weitzman"*.

Another point can be addressed concerning this MIT-style : MITItrained theorists as well as applied economists. Robert Solow himself was one of those applied economists and it is clear that his theoretical work was highly influenced by his work relationship with other applied economists – such as Edwin Kuh. The quest for empirical accuracy is what made the art of economics go forward.

One area of economic theory that appears to have been most influenced by this MIT-style is probably growth theory.

1.2 Growth at MIT

Growth : a patriotic stake

Post-second world war period was one period of opposition between capitalism and communism. Western countries had to deal with communist USSR, more powerful than ever, whose economics seemed to grow faster than ever. As a matter of fact, growth was a central policy matter in the United States of the 1950's and the 1960's. The American Minister of Defence appealed to economists to produce studies under military contracts. Those studies would feed the economic research program of MIT. Growth economics was already an important area of research – in fact since Domar's 1939 contribution. The MIT managed to distinguish itself from other institutions on that subject thanks to that special "MIT-style". With their specific tools and pragmatic way of dealing with economic growth issues, MIT-economists appeared to be the most qualified to tackle that patriotic stake. Hence, early sixties became the "golden age of growth theory" (Stiglitz, 2001, Boianovky and Hoover, 2013). Peter Diamond's and David Levhari's PhD dissertations (resp. 1963 and 1964) both had growth – in link with public debt - at the center of their subject. The conjunction of all those reasons – historic, methodological, political – might explain the success of the 1956 Solow model. Its elegance, the relatively "simple" way – in the MIT-style – it seemed to answer the instability problem of the Harrod-Domar model allowed a specific framework, in which other growth models would be developed, to become popular.

Solow's 1956 growth model : origins

Robert Solow was 16 when he joined Harvard College in 1940. He joined the U.S. Army until he was demobilised in 1945. He then went back to Harvard and started to work under Wassili Leontief's guidance as a research assistant. Solow produced the first set of capital-coefficients for Leontief's input-output model. A connection can be made between his work on Leontief's model and the interest he took on Harrod's work. In his article "A Note on the Price Level and Interest Rate in a Growth Model" (1953-54), Solow himself identifies Harrod's dynamic model to the Leontief's input-output model. In a footnote of this article, he also connects Domar's model to Leontief's (see Marcel Boumans, 2009) :

"The same kind of theoretical system was formulated quite explicitly by Evsey Domar in "Capital Expansion, Rate of Growth, and Employment," Econometrica, April, 1946, p. 137, and subsequent papers, where the treatment is even more clear-cut than Harrod's. I refer to Harrod instead only because I here follow Harrod in the use of discrete time periods."

1956 Solow's paper is an attempt to solve the inherent instability problems of the Harrod-Domar model. Even if Solow points out, in the introduction of the article, the fact that *"A remarkable characteristic of the Harrod-Domar model is that it consistently studies long-run*

problems with the usual short-run tools", he cannot accept so much instability for a simple reason : the Western economies don't show such signs of instability, therefore this model cannot describe reality and another one must be invented. Solow sees the instability of the Harrod model as the result of a dubious assumption : production takes place under fixed factor proportions. That leads to the fundamental opposition between the warranted rate of growth (determined as the ratio of the savings rate and the capital-output ratio) and the natural rate of growth (determined by the rates of technical progress and population growth) which allows the economy to be *"at best balanced on a knife-edge of equilibrium growth"*. As Solow explains to us : *"Were the magnitudes of the key parameters – the savings ratio, the capital-output ratio, the rate of increase of the labor force – to slip ever so slightly from dead center, the consequence would be either growing unemployment or prolonged inflation."* His solution is then to abandon the assumption of fixed factor proportions, the instability problems disappearing with it. The model he presents seems to work : it runs smoothly and the warranted rate of growth equals the natural rate of growth in the long run.

Solow's 1956 growth model : a quick presentation

The world famous model is the perfect example of the MIT-style way of doing economics. Solow uses a relatively simple dynamic model to study the general conditions of growth. The roots of the use of those mathematical models can be traced back to Samuelson's contributions from the second part of the 1930's and his work for the RAND corporation in 1949. As this model – that we shall call 'neo-classical' from now on - is largely known, I will present its main features, so that I will be able to discuss its consequences in comparison with 'post-keynesian' models produced in Cambridge (UK).

Solow's model is based on a production function, with substitutability of factors, written as follows :

$$Y = F(K, L) \tag{1}$$

with K : capital and L : work

The marginal productivities of each production factor are positive and decreasing. Inada's assumptions (à préciser en pied de page) are respected. Through a definition of savings S that always equals investment I – putting aside a central Keynesian assumption about effective demand – Solow manages to derive the fundamental dynamic equation of growth :

$$S = sY = sF(K, L) = \dot{K} = \frac{\partial K}{\partial t} = I \tag{2}$$

Here capital depletion is not taking into account.

$$y = f(k) \tag{3}$$

with $y = \frac{Y}{L}$ and $k = \frac{K}{L}$

$$\frac{\dot{K}}{L} = sf(k) \tag{4}$$

$$\frac{\dot{k}}{k} = \frac{\dot{K}}{K} - \frac{\dot{L}}{L} = \frac{\dot{K}}{K} \breve{} n \tag{5}$$

with n : growth rate of L

$$\dot{k} = sf(k) - nk \tag{6}$$

Here is our dynamic equation of growth. Capital per capita variation $\dot{k}$ depends on the level of n relatively to the level of f(k). The condition of equilibrium is :

$$\dot{k} = sf(k^*) \breve{} nk^* = 0 \tag{7}$$

So we have : k* such as $sf(k*) = nk \rightarrow sf(k^*)/k^* = n$.

Dynamics and distribution

Solow's model shows an incredible stability of the growth path described.
If $k < k^*$, there is $sf(k)/k > n$: K increases faster than L
In this case, the system is such that $\frac{\dot{K}}{K} = \frac{I}{K} = g > n$.
K becomes more abundant than L : its price decreases relatively to L price. Producers then prefer K to L : the ratio k increases until it reaches k^*
If $k > k^*$, the results are symmetrically the same : the ration k decreases until it reaches k^*.

The system equilibrium is such that $\dot{k} = 0$ so we have :
$\frac{\dot{k}}{k} = \frac{\dot{K}}{K} - \frac{\dot{L}}{L} = \frac{\dot{K}}{K} - n = 0$
$\rightarrow \frac{\dot{K}}{K} = \frac{\dot{L}}{L} = n$
And : $\frac{\dot{y}}{y} = \frac{\dot{k}}{k} = 0$
$\rightarrow \frac{\dot{y}}{y} = \frac{\dot{Y}}{Y} - \frac{\dot{L}}{L}$
$\Rightarrow \frac{\dot{y}}{y} = \frac{\dot{Y}}{Y} = \frac{\dot{L}}{L} = n$

The Cobb-Douglas function is the typical production function that meets the conditions imposed by the model :
$Y = K^{\alpha} L^{1-\alpha}$
From the calculation of the marginal products of capital r and of work w, we find the following distribution of revenue : $\alpha = \frac{rK}{Y} = \frac{P}{Y}$ and $1 - \alpha = \frac{wL}{Y} = \frac{W}{P}$ where P : sum of profits and W : sum of wages.

Distribution is fixed through time, pre-determined by the model. Indeed, P/Y and W/P are

constant and L and K increase at the same rate n. Solow has resolved the Harrod-Domar instability problem, so it appears. This presentation is obviously a very brief one and I shall discuss further the link of the model with the Harrod-Domar model in the next sections. In his 1956 contribution, Solow discusses the behaviour of interest rate and wages and he adds a term of neutral technical progress A in the production function such as :

$$Y = A(t)F(K, L)$$

The results are similar : the growth path is stable, depending on the rate of growth of the population. In 1957, Solow went further with an empirical work based on U.S. Data from 1909 to 1949. Around the same time, Solow and Samuelson were working on linear programming for economics, allowing the analysis of efficient accumulation paths. In 1958, they released a book, in association with Robert Dorfman from Harvard University on that subject. Tools and theory were developed side by side and it tourned out to be a success for the MIT-style.

This specific identity of the MIT-economists, that I attempted to describe, can be grasped and defined – maybe more easily – through the interactions that the MIT-community had with other intellectual community.

In the US, MIT's principal "enemy" was probably the Chicago monetarists, who considered Solow and Samuelson as neo-Keynesians : after all, the MIT-economists never denied Keynes legacy and his ideas of flawed markets. But during the period that we are looking at, MIT's real "nemesis" were the Keynesian economists from the old continent. Their theoretical cousins were more than sceptical with the way growth was tackled by the "neo-Classics".

2 Cambridge, UK : protectors of the Keynesian heritage ?

2.1 A very brief intellectual history and its protagonist

Cambridge : An ancient community

Cambridge is known to be the university were the first three-year course was taught since 1903. This achievement is the logic consequence of the rise of the teaching of economic principles during the 19^{th} century. Alfred Marshall (1842–1924) was Professor of Political Economy at the University of Cambridge from 1885 to 1908 and founded the Cambridge School of Economics. He intended to make economics an independent subject from other social sciences. Marshall taught a whole generation of English economists that would be moulded by his methodology. John Maynard Keynes studied under his guidance and Keynes' work is heavily influenced by this Marshallian heritage, which will be carried on by Keynes's contemporaries and successors such as J. Robinson, Kaldor, Kahn, Meade, etc. Cambrigde (with the LSE and Oxford) was at the centre of the development of economics in the first part of the$XXth$ century.

Post Keynesianism : origins

The English Cambridge was made up of various school of thoughts. One of those distinguished itself from the other in the 1950's and the 1960's : the Post-Keynesians. Their origins can be traced back to the publication of Keynes's 1936 General Theory of Employment, Interest and Money (GT from now on). Basically, they did not agree with the further interpretation of Keynes's GT made by the author of the neoclassical synthesis realised with the IS-LM model by J. Hicks, R. Meade, and others. The Post-Keynesians, that could be found in Cambridge (UK) and in the USA saw this synthesis as a treason against Keynes's true message. As a matter of fact the early Post-Keynesian representatives – Nicholas Kaldor, Joan Robinson, Richard Kahn and Pierro Sraffa (all in UK) – directed their first criticisms towards the Solow growth model. They could not accept a model in which full employment is ensured thanks to perfect factor substitution and an aggregate production function, which happens to behave exactly as expected by the model and its author. This approach of long-run economics did not correspond to what Keynes might have wanted to express. That is how the 'Cambridge Capital Controversies' began in the early 1950's and continued in the 1960's, ending with the apparent analytical victory of the Post-Keynesians. The Post-Keynesians tried to deal with questions that Keynes did not himself explore such as income distribution or economic growth. To do so they put the concept of effective demand at the heart of their analysis, even at the long-period. As J.E. King defines the main features of Post-Keynesianism, in his article from the Palgrave Dictionary, as such :

"For Post Keynesians, demand constraints upon output and employment are not restricted to short period and are not the result of market imperfections or wage and price rigidities, but must be explained instead in terms of the characteristics of money and the pervasive influence of fundamental uncertainty."

That explains their rejection of IS-LM : with uncertainty and animal spirits, you can't have a stable IS curve, nor a working LM function with endogenous money. Beyond that, Post-keynesians affirmed themselves through methodological and theoretical specific positions in comparison with the mainstream economics, that MIT-style partly represented.

An alternative to the 'neoclassical orthodoxy' of MIT

In many ways, Post-Keynesians looked like the MIT-economists. They both aimed at expanding and exploring the leads left Keynes, the difference lying in the fact that the Post-keynesians just did not accept the methodological and theoretical presuppositions that the MIT- style imposed. As a matter of fact, they were – like Solow and Samuelson – in favour of government intervention, but not for the same reasons.

They rejected the Phillips Curve model – linking level of wage inflation and unemployment : they believed in a more socio-political analysis of the problem of unemployment in which distribution conflicts were central. In the same way, they emphasized the role of savings propensities in their macroeconomic model, instead of resorting to the marginal productivity theory of income distribution, which they dismissed through the Capital Controversies.

The Post-Keynesians shared Keynes's distrust toward too much formalized economics. J. Robinson is known for her methodological heterodox position : she was highly sceptical of the relevance of the use of equilibrium models. There were there no room for history in those logical time based models.

Like the Solow model, the Post-Keynesian growth models find their roots in the Harrod-Domar model but their approach is radically different but not in a single way. As a matter of fact, Kaldor himself was criticised : his basic assumptions were not really Keynesian any more, for he abandoned the demand effective hypothesis in some of his growth models .

2.2 Post Keynesian growth models

As I explained in the previous section, demand is central for the Post-Keynesians. That is why they developed a certain number of demand-driven growth model. Those models featured the principle of effective demand and did not impose full employment of resources. As Robinson (1956) pointed out, it is only during a mythical "golden age" that the conditions of full-employment of factors and growth path stability can be met. Several economists faced the challenge of trying to extend Keynes's logic to the long term, while respecting the main features of the GT : Robinson, Kaldor, Pasinetti, Michal Kalecki, Edward Nell, among others. I shall explore some examples of growth models developed in Cambridge UK during the 1950's and the early 1960's, that can be seen like a response to the Solow model and the success that it gained throughout the world. Those models singularised themselves from the Solow model by using concepts other than the theory of marginal distribution, such as those of savings propensities and investment functions.

Kaldor (1956) : growth and distribution

Kaldor presented his model in a 1957 paper. Just as Solow, Kaldor is heavily concerned with the Harrod-Domar instability problems and exposes his own solution, that goes through the use of a distribution model. Here are few details about the model :

$$Y = W + P \tag{8}$$

the national revenue is divided between wages and profits

$$S = S_w + S_p \tag{9}$$

total savings are made of savings from workers and savings from capitalists

$$S_w = s_w W \tag{10}$$

with s_w : worker propensity to save

$$S_p = s_p P \tag{11}$$

withs_p : capitalists propensity to save

Those behavioural equations respect Keynes's 1936 assumption. From the basic savings/investment identity we obtain :

$$I = S \tag{12}$$

$$I = swW + s_p P = s_w(Y - P) + s_p P = (s_p - s_w)P + s - wY \tag{13}$$

$$\frac{P}{Y} = [1/(s_p - s_w)]\frac{I}{Y} - \frac{s_w}{(s_p - s_w)} \tag{14}$$

Profits are positive when the workers' savings propensity are low, when compared the capitalists' savings propensity : $s_p > s_w$. And we can deduce from the fact that if $\frac{I}{Y} = s_p$ then $\frac{P}{Y} = 1$ and $\frac{W}{Y} = 0$ (and conversely if $\frac{I}{Y} = s_w$, then $\frac{P}{Y} = 0$ and $\frac{W}{Y} = 1$, so we have :
$s_w < \frac{I}{P} < s_p$

Let's resort to variables defined by Harrod in his model :

- $g_n = \dfrac{\dot{Y}}{Y}$ is the natural growth rate of the model
- $v = \dfrac{\dot{K}}{\dot{Y}} = \dfrac{K}{Y}$ is the marginal coefficient of capital (which is a constant).

Using the fact that $g_n v = \frac{I}{Y}$, we can rewrite equation (2.7) :

$$\frac{P}{Y} = \frac{(g_n v \breve{} s_w)}{(s_p - s_w)} \tag{15}$$

$$\frac{P}{K} = (\frac{1}{s_p} - s_w)\frac{I}{K} - \frac{s_w}{(s_p - s_w)v} \tag{16}$$

$$g_n = \frac{[(s_p - s_w)\frac{P}{Y} + s_w]}{v} \tag{17}$$

As$\frac{P}{Y}$ has to be between 0 and 1, we deduce from $0 < (g_n v \breve{} s_w)/(s_p - s_w) < 1$, that the rate of growth is stable and allows full-employment, as defined by the Harrod-Domar model, when :

$$\frac{s_p}{v} > g_n > \frac{s_w}{v} \tag{18}$$

Kaldor refines g_n's interval, using concepts of minimum wage and minimum profit. The share of profits depends on the values of both savings propensities and the maximum and minimum value of $\frac{P}{Y}$. Kaldor obtains the following equilibrium condition :

$$\frac{[a(s_p - s_w) + s_w]}{v} \geq g_n \geq \frac{[vr_{min}(s_p - s_w) + sw]}{v} \tag{19}$$

with $a = 1 - W_{\frac{min}{Y}}$ and r : rate of profit

Kaldor model seems stable as long as $s_p > s_w$ and profit margins depends on the difference between demand and supply.
If $I > S$, the system will come back to $I = S$

A quick overview on Robinson (1962) and Pasinetti (1962)

Joan Robinson concentrated her efforts on the relationship between accumulation and profit. She based her accumulation model on the fact that the relations between the variables have to be specified and so the motives of the human behaviours. Joan Robinson is thorough in the description of the basic premisses of her model (technical conditions, investment, etc) which are largely commented in the literature (Charles, 2006), and I will focus on the savings conditions. Robinson chooses to adopt a "hard" kaldorian saving function with sw = 0 (the workers consume all their revenue) so S = spP. She defines the rate of macroeconomic accumulation as such :

$$g = f[(P/K)^a] = f(r^a) \tag{20}$$

with $f' > 0$ et $f'' > 0$.

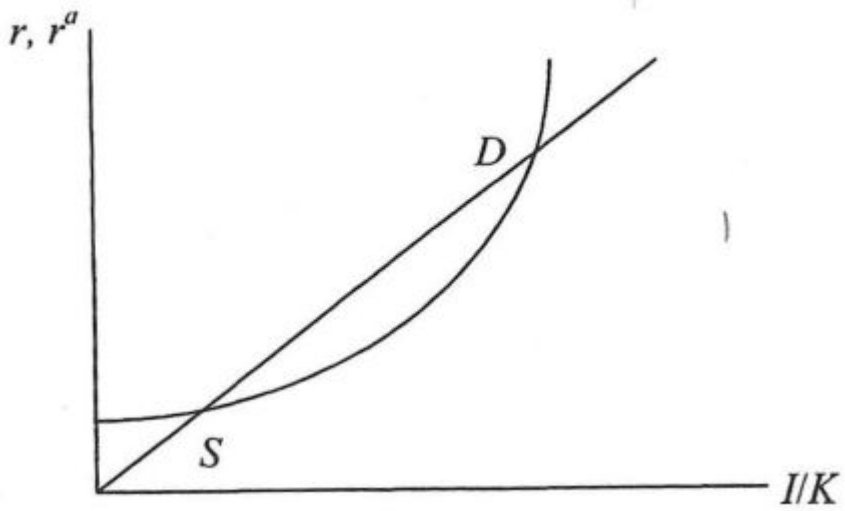
The rate depends on the anticipated rate of profit. She places the firms' desire to accumulate capital at the centre of her model. There is a double relationship between the accumulation rate and the profit rate. As a matter of fact, the effective rate of capital accumulation gives the amount of profits :

$$r = \frac{P}{K} = \frac{I}{K}\frac{1}{s_p} = \frac{g}{s_p} \tag{21}$$

with $s_p = 1$

That allows us to draw the following curves :

Figure 1.8 – Les équilibres dans le modèle de Robinson



We see that there are two equilibrium : one is stable, the other is not.

Robinson's model is quite interesting because it highlights situations of potential disequilibrium in which savings and expectations have leading roles. But I shall come back on that aspect of the model in a few sections.

Luigi Pasinetti proposes an improved Kaldorian model, that takes into account the fact that workers gain also profits from their savings – a thing that Kaldor forgot in his model. That modifies the profit and saving equations :

$$P = P_w + P_c \tag{22}$$

and the previous equation $S_w = s_w W$ becomes :

$$S_w = s_w(W + P_w) \tag{23}$$

$$S_c = s_c P_c \tag{24}$$

then we have :

$$I = \dot{K} = s_w(W + P_w) + s_c P_c = S \tag{25}$$

I shall restrict myself to the most important assumptions and results of the model, details being provided in the adequate literature. Just like Kaldor, distribution of revenue is socially based : it is the distribution of revenue between workers and capitalists rather than between profits and wages that is observed. In the long term, Pasinetti assumes that the capital stock of each class grows proportionally to their respective savings : both classes have the same wealth growth rate (if not, one of the classes would disappear in the end).

Pasinetti manages to show that his model is stable. The main observation that can be drawn out this model is probably the fact that Pasinetti ends up with a rate of macroeconomic profit

doesn't depend at all on the saving propensity of the working class. The rate of profit is defined as following :

$$\frac{P}{Y}\frac{1}{s_c}\frac{I}{Y} = \frac{g_n v}{s_c} \tag{26}$$

The higher the level of investment is, the higher the rate of profit. If the capitalists' propensity to save decreases then the profit rate increases. It is kind of a way to come back to Keynes's GT analysis : investment is what makes the economy work. Just like Malthus emphasised, consumption from non-working class has an important role in maintaining the stability of the system.

In some ways, the reflections led by Pasinetti can be linked to those of Michal Kalecki in 1942 and in his later models which focused, amongst other issues, on the connection between wages, consumption and aggregate demand.

3 Two intertwined interpretative communities : seeing and trying to solve growth models weaknesses

In the following part of my paper, I will try to depict the relationship between those two intellectual communities. As a matter of fact, it is through the confrontation of ideas from both ends that the growth debate evolved. And despite their ontological disagreements, the members of the two communities held fruitful correspondences and exchanges.

3.1 A complicated relationship

Keynesians from both sides of the ocean had quite a complicated relationship. They were like cousins that were raised in different ways, yet having a common ancestor. In the US, Solow and Samuelson were considered Keynesians – above all by their monetarist 'enemies' – but for their British cousins they had betrayed Keynes in spirit : there methods and theories were not in phase with Keynes's original message anymore.

A tradition of visiting and corresponding

Paul Samuelson spent some time in Oxford in 1949. It was kind of tradition for MIT to send some of their students to England. Cambridge UK welcomed Solow in 1962 but also Fisher and Stiglitz and some graduate students in 1965 (Diamond went there in 1967). Frank Hahn visited the MIT in 1956-57. Amartya Sen and Joan Robinson were there in 1961. There is a significant amount of correspondences between the members of the two communities. Those correspondences show their concern for the questions of the moment during the 1950's and the 1960's : technical progress, aggregation of capital inputs in production functions, the shape of production functions, the possibility of production technique reswitching, etc, were the subject of a *"raging war"* (Cherrier, 2011) between Hahn, Robinson, Kaldor (who did not all agree with one another) on one side and Solow and Samuelson on the other. The complicated aspects of

those relationships can be seen in the following quotation of a letter of Solow to Sen :

"I'm afraid I got a little annoyed in Cambridge last year by the indiscriminate use of "Keynesian" as an adjective meaning "mine" and "neoclassical" to mean "yours." To the extent that "neoclassical" describes the belief that a capitalistic economy tends automatically to full employment, I am not neoclassical and neither is James Meade. To the extent that "neoclassical" means a belief that you need a plausible microeconomics that allows for some kind of near optimizing behavior under whatever market conditions prevail, then it is not in the slightest incompatible with being Keynesian (10/26/64, Box 60 folder S7)."

Some renegades

The division in camps was not as simple as a UK vs. US partition. There were in MIT, some economists who would be more inclined to agree with the hard British Post-Keynesians. Robert Eisner (1958) was one of those American economists who, like the British Keynesians, did not agree with Solow, James Tobin and the "neo-classical" interpretations of Harrod and Domar (Hagemann, 2009). In the same way, in the UK, Meade (1963) and then Meade and Hahn (1965) were highly critical at the Pasinetti model :they tried to show that this Post-Keynesian model was only a specific case of the traditional "neoclassical" framework. Yet Hahn was not as satisfied as we could think with the Solow model : he would be the first to point out its limits (1960, 1964). Those cases symbolises the difficulty to isolate each community under a unique identity.

3.2 Different cultures of understanding

Interpretative communities

So we have two intellectual communities with obviously the similar theoretical roots, the same preoccupations and an utter respect for each other. Yet, they do not seem to be able to agree on many things. Beyond the spatial separation, they appear to have evolved – since the Second World War – as though they do not speak the same language. Their methods, their way of expressing themselves and the concepts they think as central to understanding economic issues are merely different. If we use the concept of interpretative community developed by Stanley Fish (1980), that might explain why the two communities were unable to accept each other's interpretation of the growth economics issues.

"Last time I ended by suggesting that the fact of agreement, rather than being a proof of the stability of objects, is a testimony to the power of an interpretive community to constitute the objects upon which its members (also and simultaneously constituted) can then agree. This account of agreement has the additional advantage of providing what the objectivist argument cannot

supply, a coherent account of disagreement. To someone who believes in determinate meaning, disagreement can only be a theological error. The truth lies plainly in view, available to anyone who has the eyes to see ; but some readers choose not to see it and perversely substitute their own meanings for the meanings that texts obviously bear. [...] In the view that I have been urging, however, disagreements cannot be resolved by reference to the facts, because the facts emerge only in the context of some point of view. It follows, then, that disagreements must occur between those who hold (or are held by) different points of view, and what is at stake in a disagreement is the right to specify what the facts can hereafter be said to be. Disagreements are not settled by the facts, but are the means by which the facts are settled. Of course, no such settling is final, and in the (almost certain) event that the dispute is opened again, the category of the facts "as they really are" will be reconstituted in still another shape." (*Is There a Text in This Class*, p.338 – 339, 1980)

Just as Solow could not accept an interpretation in terms of distribution, Robinson completely rejected the marginalist approach of the MIT. When we have in mind Fish's perspective on those rather gregarious behaviours, the different interpretations of the Harrod-Domar model that resulted are now understandable. But as I emphasised in the previous section, the relationship between those two communities is much more complicated than a simple opposition. Some of their members placed themselves in some kind of in-between position – such as Amartya Sen and Frank Hahn (but I will concentrate on their case in the following section), trying to link the two conceptual frameworks, probably as a way to find new leads to overcome the theoretical obstacles that each community has ended up meeting.

Harrod-Domar : truly a so important misunderstood ?

When one tries to take a good look the history of growth economics, there is one point in this story that appears to be the focal point of all that followed. One cannot talk about the Solow model without mentioning the Harrod-Domar model (I could not dodge the ball either). What is actually funny is that it is not the Harrod-Domar growth model itself that changed everything, but rather the interpretation – or for some the misinterpretation – that was made of it. First common mistake that has been made was probably to almost completely assimilate Domar's model to Harrod's. As Hagemann (2009) shows us, even if the two models are similar in some aspects – for many that mistake is mostly due to similar notations – they do not really fill the same purposes. Harrod's model distinguishes itself from Domar's through the use of an investment function of the multiplier type. Hagemann (2009) perfectly summarises both models :

Table 1 Domar

	Flows	Stocks
Income effect	$\dot{Y}_D = \frac{1}{s}\dot{I}$	$\dot{K} \equiv I = S = sY_D$
Supply-side capacity effect	$\dot{Y}_{cap} = \frac{1}{v_{cap}} I$	$Y_{cap} = \frac{1}{v_{cap}} K$
Equilibrium condition	$\dot{Y}_D \stackrel{!}{=} \dot{Y}_{cap}$	$Y_D \stackrel{!}{=} Y_{cap}$
Solution	$\frac{\dot{I}}{I} = \hat{I} = \frac{s}{v_{cap}} = g$	$\frac{\dot{K}}{K} = \hat{K} = \frac{s}{v_{cap}} = g$

With s : constant average and marginal propensity to save
$S = sY$
v_{cap} : capital-output ratio in growth equilibrium (full-capacity utilization and full employment).
Y_{cap} : full-capacity output,
Y^D : aggregate demand, and the
operator x^ : growth rate of a variable x.

Table 2 Harrod

	Flows	Stocks
I-S-Condition (= capital accumulation)	$I \stackrel{!}{=} S = sY$	$I = \dot{K} = sY$
Investment function/ warranted capital stock	$\dot{K} = I_w = v_w \dot{Y}$	$K_w = v_w Y$
Solution	$\frac{\dot{Y}}{Y} = \hat{Y} = \frac{s}{v_w} = g$	$\frac{\dot{K}}{K} = \hat{K} = \frac{s}{v_w} = g$
	$\hat{I} = \hat{Y} = \hat{K} = \frac{s}{v} = g$	

$I_t = v_w(D_t \breve{} Y_{t-1})$
where D_t : expected demand for overall output
$Yt-1$: actual output in the last period
v_w (acceleration coefficient) : desired capital-output ratio
For Harrod (1939), his model is the "marriage of the 'acceleration principle' and the 'multiplier' theory". And there will be : $Y_t = \frac{1}{s} I_t$

Even if similar, the two economists did not have the same ambition while creating their model, Harrod having a clear interest in the question of cycles, much more than Domar. But basically, the models have the same conclusion – that probably participated to the process of assimilation of one to the other : growth is inherently unstable in this model. Three types of growth rate

are defined by Harrod : the natural growth rate, the warranted growth rate and the actual growth rate. The warranted – warranted by the state of the system – rate of growth has every chance to be different from the natural growth – long-run equilibrium growth rate – and any tiny deviation from the equilibrium growth leads the system to a spiral of inflation or unemployment. In the 1950's, growth seemed quite stable : there had to be a way to make the natural rate of growth and the warranted rate of growth correspond. And that is exactly what Solow and Kaldor tried to do : they solved the problem in their own way, according to their respective community of understanding. Once again, let's use another nice diagram from Hagemann (2009) :

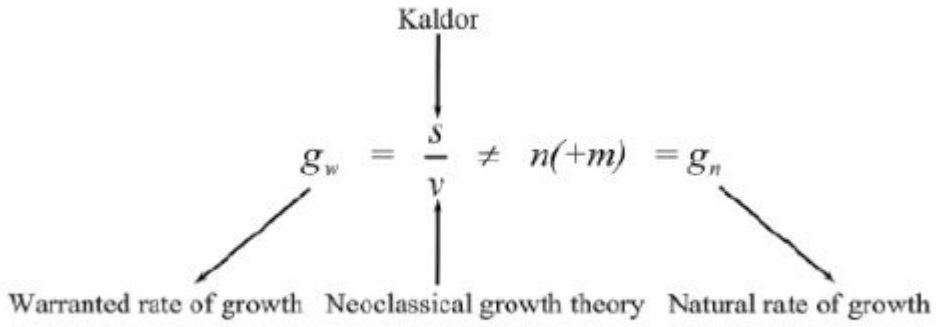


Figure 1 Potential solutions for the secular instability problem

As we can see here, Kaldor took interest in the saving propensities (s) aspect of the problem and Solow took care of v – the capital-output ratio of the system : they both oriented their analysis according to their community of understanding.

Even today, the interpretation of Harrod's works is problematic. Did he really try to extend to the long run the Keynesian framework ? For a lot of historians of economic thought, the answer is yes. But for some, Solow simply went completely wrong going along this path of interpretation. Not because his interpretation is wrong, but because the questions that he presents in his 1956 contribution are simply not the good questions (Halsmayer and Hoover, 2009). His famous "knife-edge" problem just does not exist. The main purpose of Harrod's was not even the long run growth equilibrium : after all, he doesn't tackle the subject of natural rate of growth until quite late in his contribution. His main attention is on the question of dynamics and the importance of the difference between the warranted rate of growth and the actual – expectations dependant - rate of growth – which is itself not easily defined up to now. And as we can see in the wonderful Hagemann's diagram, the actual rate of growth is not included in the attempt to cope with the secular instability problem. Does that mean that Solow – and by extension Kaldor and all both communities went completely wrong ? No, I would support that those contributions were their way to express their mutual interest for long and short run analysis, and that according to the way of each community. Those intellectual works are part of a greater intellectual process. As a matter of fact, those weaknesses would soon be identified by both the communities and would be addressed in the following years. I shall expose that aspect

of the history of the growth models in a few sections.

4 Some tentative to link our two communities together : synthesis and the road to medium-run equilibrium

As I supported a few sections below, I think those debates, mistakes, interpretation and misinterpretation of the issue of growth were part of a bigger intellectual process. That process, which not a conscious one nor a constant one and is the result of the interactions of different minds, appears to have at its centre the purpose to unite short run and long run analysis of growth.

4.1 Identifying the weaknesses : the status of expectations and investment in growth

Hahn and Matthews (1964), later Sen (1970), pointed out that Solow's success is linked to the fact that he dismissed a part of Harrod's analysis of growth : Solow doesn't take into account the instability problem related to the potential divergences between warranted rate of growth and actual rate of growth. By doing so, Solow did not place his model in the same framework as Harrod's and excluded all possible analysis of cyclical fluctuations. Those divergences, which Harrod related to expectations, were at the centre of Harrod's attempt to have a model which explained economic growth and cycles at the same time. In 1966, Solow tried to clear his position, acknowledging that he did ignore a part the instability problem but still maintaining that disequilibrium was a matter of keynesian short-run analysis :

"It is clear to me that I oversimplified matters in 1956. The model was new and I didn't understand all its implications. Some of what Harrod called instability is, of course, a matter of the behavior of effective demand, off equilibrium paths. Harrod never specified very clearly what he had in mind, and indeed there is very little in the literature even now that marries the theory of growth and effective demand. What I was getting at in 1956 was this : the special character of Harrod's model rests in the fact that the natural and warranted rates of growth are independent numbers... That characteristic of the model rests on fixed proportions. (It is immaterial whether Harrod believed that factor proportions are technically fixed or simply never change.) In turn, at least some aspects of "instability" arise because the economy is always being pulled away from the warranted path because it differs from the natural path."

Solow's 1956 model main purpose was indeed to trace full-employment – not actual – paths towards the natural steady state. Solow received the support of Evsey Domar, who acknowledged the 1956 model just before joining Solow at MIT, where he became his colleague. Yet it is not like the question of expectations had never been discussed. In a 1952 article, Hahn addresses the issue of expectations and conditions of equilibrium. He tries to define the type of expectations

that would assure a dynamic equilibrium. In his 1953-54 contribution on price levels and interest rates, Solow himself tackles the issue in the conclusion of his article :

"From another angle, the equations do not reflect the fact that one could affect output by operating on prices. This would appear naturally in an attempt to build up a causal dynamics. A mechanical first step in this direction could be made by letting the choice between consumption and investment depend on the interest rate and price level in some arbitrary but simple way. I do not carry this out here, although the formal execution would be fairly easy, because I am not convinced that this obvious line of approach is necessarily the best one. Another possibility would be to think of investors as Ramsey-type utility maximisers over time. A certain amount of depth could be added by assuming the existence of two goods differentiated with respect to their utility in consumption and in investment. But this goes beyond the simple Harrod model and past the bounds of simple arithmetic."

Solow mentions what appears to be an independent investment function but did not go further in his reflections for the reason he evokes. We could here a sign of the weight of the weight of the community surrounding the process of models developing.

Let us also recall the Robinson model that I presented earlier : it was an anticipations based model that illustrated situations of equilibrium and disequilibrium, using the mechanism of entrepreneurs' anticipations. That aspect did not really have any influence in the following Post-Keynesian work on growth. Robinson's contribution was in fact eclipsed by Pasinetti break trough. The Post Keynesian community did not acknowledge Robinson's work : that also testifies of the importance of the community of understanding in the process that leads a concept on the top priority list of the researchers.

That being said, does that mean that nothing were done about the question of expectation and investment ? Of course not. Those issues kept coming back in the debates, in various way : the possibility to think a growth model that featured both long and short characteristics, equilibrium and out-of-equilibrium outcomes started to work its way through the discussions.

4.2 Frank Hahn and Amartya Sen : slowly constructing the premisses of a synthesis

4.2.1 Amartya Sen : above the battle

In 1962, Amartya Sen presented 'The Money rate in the pure theory of growth' at the International Economic Association Round-Table Conference on the Theory of Interest Rate, held at Royaumont, in France (Sen, 1970). There underlined the difficulty to define an autonomous investment function for a growth model that would reflect the empiric phenomenon. He also pointed out the lack of relevance of the assumption of perfect foresight : that would reflect in

his following contribution.

A "fun" comparison between Neo-Classical and Neo-Keynesian models

So, in 1963, Amartya Sen published an article where he proposes to discuss the different aspects of the 'neo-classical' (NC) and 'neo-keynesian' (NK) models of distribution. That article is interesting at several level. First, it testifies of the quite complex relationships between the two communities. Sen spent some time at the MIT in 1962. Obviously, he has been quite puzzled by the debates between the two communities. Maybe those are more alike than they think themselves or at least their share as much defaults ? Second, Sen chooses to analyse the different distribution model : that reflects the importance of the concept of distribution in the growth debates. Third, in this article, Sen touches upon the subject of anticipations – a subject that will be higly relevant in the following discussions. At last, that article has been heavily commented by Paul Samuelson in a letter he sent to Sen in September 1963 (Solow, box 68, Duke University archives).

The model

To be able to compare the two distribution models, Sen uses a simplified model from which he makes the features vary to correspond to one or the other model.
Here are the main assumptions of Sen's simplified model :
- only one good X, that can be consumed or use in production ;
- the production function displays constant return to scale ;
- there is no problem of capital depletion : each year a fixed proportion of the capital goods is discarded so the value of the capital goods are independent from their age ;
- in the short-term, capital and labour supplies are given and they are the only production factors. Investment at year n, doesn't yield anything until year n+1 : the capital stock supply is given through what has been left from the previous year. For simplicity, Sen chooses an inelastic labour supply ;
- saving behaviours depend on the source of the revenue : if a worker gains a part of his revenu from the property of something, then he will behave towards this part of his revenue just like a capitalists towards his profits.

Notations :
X : net flow of produced goods
$\bar{X}$: stock of goods used in the production
L : quantity of labour used in the production
w : wage rate in terms of X
π : profits in terms of X

s_p : propensity to save out of profits

s_w : propensity to save out of wage

I : investment in terms of X

$$X = X(L, \bar{X}) \tag{27}$$

with a production function homogeneous of degree one

$$w = \frac{\partial X}{\partial L} \tag{28}$$

$$X = \pi + wL \tag{29}$$

$$I = s_p\pi + s_w wL \tag{30}$$

$$I = I^* \tag{31}$$

with I* being the investment level corresponding to a given behaviour (Keynesian "animal spirits") and to the level of growth expected by the entrepreneurs.

Using the following notations, Sen presents us the main features of the model.

Sen points out that this system can't satisfy all its equations and the full employment conditions L=L* and K=K*. Indeed, there are five equations but only four unknowns X,w,π and I. It is only by accident that a solution can be found in this case. To resolve the system, you must abandon one equation or add an unknown. That is what NK and NC theories do in their specific way.

Sen finds his way back to the NC system by dropping the equation (31). Investment is not arbitrarly given and it is equalised with savings, through an out-of-the system mechanism : the interest rate. We then have I = S. That mechanism is not discussed by Solow nor by Swan : none of them used an independent investment function. In James Meade's analysis, that mechanism is not automatic : there is a central monetary authority that will assure the equality between S and I.

Sen then drops the equation (28), resolves the system to find the NK results :

$$\frac{\pi}{X} = \frac{I^*}{X(s_p - s_w)} - \frac{s_w}{(s_p - s_w)} \tag{32}$$

with $s_p \geq \frac{I^*}{X} \geq s_w$

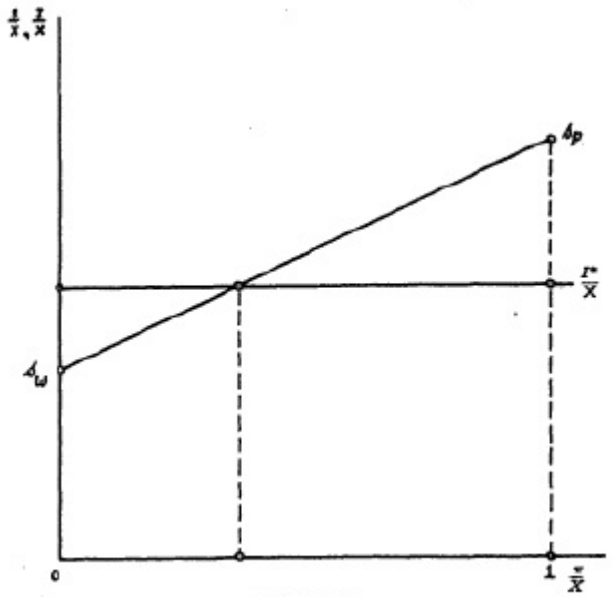


DIAGRAM 1

$s_w.s_p$: share of the savings in the total income. The central point gives us the income distribution between labour and capital.

Sen goes through the same process by dropping equation (30) : he finds the Johansen model developed by Lief Johansen in *A multi-sector Study of Economic Growth* (Amsterdam, 1960), where the I/S equality is maintained thanks to taxations and allocations. Finally, Sen adds unemployment as a new unknown to be in a General Theory-like model.

What happens if...

As Sen underlines it, dropping (28) or (31), or (30), is in fact quite an extreme assumption. So proposes to look for the conditions for which NC and NK models can be recovered without ignoring the previous assumptions. To do so his presents three models with specific assumptions.

Model I :
- full-employment ;
- the Investment is fixed in real terms ;
- the actual price can differ from the anticipated price, such as over-determination of the model is avoided ;
- the level of price p is unknown ;
- q : level of price anticipated by the employers
- m : money wage rate, determined according to q through a process of competition that leads to full-employment.

The equations of the model are :

$$X(L^*, \bar{X}^*) \tag{33}$$

$$m = \frac{\partial X}{\partial L^*} q \tag{34}$$

$$R = Xp - L^* m \tag{35}$$

R : monetary profits p : determined by investment/savings process such as $S = I$.

$$I_p = s_p R + s_w L^*.m \tag{36}$$

$$I = I^* \tag{37}$$

Sen add a given anticipated level of price :

$$\bar{q} = q \tag{38}$$

So there are 6 equations and 6 unknowns : X, m, p, q, R, I.
Sen shows us quite easily that the NK model of distribution results can be recovered :

$$\frac{R}{pX} = \frac{I^*}{X(s_p - s_w)} - \frac{s_w}{(s_p - s_w)} \tag{39}$$

In the same way it appears that the NC results only hold if $p = q$, yet we obtain the following condition :

$$I^* = s_p[X - \frac{\partial X}{\partial L^*} L^*] + s_w(\frac{sX}{\partial L^*})L^* \tag{40}$$

(40) is independent of the expected level of price. So if this result is true for one expected level of price, it is true all the other level. As Sen put it : *"The assumption of 'perfect foresight' cannot be made consistent with the system by varying the level of expected price. It should also be noted that the required condition for NC result to hold as given by equation (40) is that the independently determined amount of investment (I^*) must equal what the total planned saving would be if the NC distribution model held. This can take place only accidentally, and but for that NC result is invalid."* (40) is very important. If it does not hold, Sen notices that it would be quite easy to build a model with continuous inflation, thanks to the use of hicksian elasticity of substitution. But he does not beyond that remark.

So we have a model where the expected price determined the monetary wage rate which determines the level of price :
$q \to m \to p$

It is through the variable m that the system adjust itself. But what would happen if we had a variable m that had an influence on $\bar{q}$, such as $m \to \bar{q}$?
q and m are both in equations (34) and (38) : they have to be consistent the one with the other. If by accident : $q = \beta m$ then (38) and the entire system do not hold any more and full-employment is no more attainable.

Sen points out another thing : from the basic assumptions – which are crucial to the model – the changes in price levels do not affect at all the savings propensities and that seems to be quite a bad empiric hypothesis. In the NK results, the feedback of the changes in the expectations of price are not taken into account : there are no discussions about the actions that could result from those changes. That implies that even if the managers try to maximise their profit (in perfect competition), they will not necessarily attain the Pareto optimum :

$$\frac{m}{p} = \frac{\partial X}{\partial L^*} \tag{41}$$

which won't be equivalent to (34)

Model II : Basically, it is the same model as the first but with a level of investment fixed in monetary terms :

$$I.p = V^* \tag{42}$$

The resolution of the system gives us the following equation :

$$\frac{R}{p.X} = \frac{(V^* - X.\bar{q}\alpha s_w)}{(V^* + Xq\alpha(s_p - s_w))} \tag{43}$$

with $\alpha = \frac{\partial X}{\partial L^*}\frac{X}{L^*}$ which the elasticity of production with respect to the level of labour at full-employment.

As Sen makes us realise : this result doesn't correspond to NC results nor to NK results. Le NK conclusion that says that the higher the rate of accumulation, the higher the share of profits is, remains true but the distribution defined here is not the one of the NK model. Besides, the result is not independent from the seize of marginal product of labour at full-employment.

The NC result is valid only if $p = q$, substituting in the other equations, Sen gets the following necessary condition :

$$\frac{V^*}{q} = s_p(X^{\smile}\frac{sX}{\partial L^*}.L^*) + s_w.\frac{\partial X}{\partial L^*}.L^* \tag{44}$$

Unlike in the model I, this condition is not independent from price expectations : *"On the contrary, for any given level of investment (in money terms), there is one level of expected prices that makes the assumption of perfect foresight consistent with the rest of the system, resulting in the validity of the NC result."* But Sen qualifies this positive statement because : *"Of course the condition p=q still makes the model over-determined, and can only be satisfied if accidentally the right price were expected, compared with the size of money investment equal to the size of what the saving would be if the NC model held".*

Model III : Here, Sen presents a "General Theoryish" model which is supposed to depict the model of the GT : the assumption of full-employment is dropped and L becomes an additional unknown and there is always perfect foresight.

$$L \leq L^* \tag{45}$$

Sen distinguishes three possible cases from model III :

1. $\bar{L} < L^*$
2. $\bar{L} = L^*$
3. $\bar{L} > L*$

where $\bar{L}$ is the equilibrium level of employment.

He then compares those possibilities with the models he presented. Possibility 1 : this case corresponds to a stable unemployment equilibrium. The Keynesian unemployment model can hold but not the NC model nor the NK model. Possibility 2 : NC and NK models van hold but also Model III. It will be a case of perfect foresight for Models I and II (p=q). Possibility 3 : Model III does not hold. I or II are the only options. If I is chosen, the NK result will hold but not the NC result. If it is II that is chosen, none of the results will hold, except if by accident the unique level of price expectations which allows the NC result to hold is held.

Conclusion

Sen concludes his article with a very lucid point of view. The discussion that he held was "aimed mainly at reducing obscurity rather than assessing validity" and the only conclusion he can really draw out of his work is that both NC and NK use very restrictive assumptions to make their textit"special results". Those distribution models are both limited and use simplifying assumptions. That is an important point because those models are the basis of a wider class of economic theories such as economic growth. Sen ends his article the following sentence, which perfectly symbolises his in-between position in the Cambridge/Cambridge debates : *"In raising questions about these distributional models, we intend to question the entire bodies of economic thinking that share, explicitly or implicitly, the assumptions of those models."*

Samuelson look on that article

The 11th september 1963, Paul Samuelson sent a letter to Amartya Sen where he largely comments on Sen's article. That letter gives us a little insight of the intellectual relationship between two economists who held different positions. Also, it offers us the opportunity to have a look at the reaction of Samuelson towards an article that highly criticises his theoretical positions and at the way he considered the British Keynesians. It is quite in a warm and friendly tone that Samuelson addresses himself to Sen. He re-writes the model proposed by Sen in his article in a slightly different form. That allows him to give his opinion on Sen's approach. He does not really seem to agree with the formal separation that Sen made between Neo-Classical and Neo-Keynesian economists :

"You lump Kaldor and Robinson together as Neo-Keynesians. They do have some affinities : both are non-neoclassical (but in different degrees, J.R being less strong in her position). But if

I were Joan Robinson or Richard Kahn, I would sue you for slander in calling the Kaldor-Full-employment doctrine Neo-Keynesian."

Samuelson points out the little mistakes than Sen did in the presentation of his model : the model misses some features to be a real full-employment distributional model. He proposes some elements to complete Sen's model. He then emphasises the fact that Kaldorian assumptions on savings behaviours are based on quite light empirical facts. And he concludes his letter with this quite funny paragraph, which reflects the tense relationship between the two intellectual communities : *"Mind you, I have said nothing favourable to a Ramsay-Solow-Tobin Full-Employment Neoclassical model, which I might call [Model] I' without specifying it. If that model, I', is unrealistic – and I could supply some evidence on that – that as such casts no credibility on [Model] III'. Rather it makes II more interesting. Kaldorian III, which is emasculated Kaldorian, remains of minor econometric interest (albeit sp = 1 and sw = 0 has little relevance for USA 1960, whatever it may have for the land of Kaleckians)."*

Let us notice the reference to the Kaleckians : obviously, Samuelson does not hold a burning passion for them either and it would be quite fascinating to understand why, but it is not the purpose of this paper.

4.2.2 Frank Hahn and disequilibrium economics

In 1960, Frank Hahn published an article in which his main focus is the stability of equilibrium - the existence of that equilibrium is admitted from the start. In the framework used, prices adjust themselves to market errors but none of those adjustments happens instantaneously. Hahn chooses a Cobb-Douglas production function.

He presents two types of models : a model A, that he calls Wicksellian, and a model B. Let us have a look to the main features of each model.

Model A : "Wicksellian"

At all time, the following equation is verified :

$$\frac{w}{p} = \frac{\partial Y^e}{\partial L} \tag{46}$$

where w : wage rate, p : price, Y^e : equilibrium level of production, L : labour

If the equilibrium is perturbed, the following chain of events takes place :

- the prices of the goods increase if there is an excess demand or decrease if there is an excess supply ;
- the distribution of income remains unaffected ;
- the expectations on the future change rate in terms of price increase ;

- that leads, with the monetary interest rate considered as given, to a substitution between K and L ;
- that substitution have itself an impact on the excess of demand of goods.

So Hahn asks himself : what process will lead back the system to equilibrium ? In fact, it depends, for any hypothesis of formation of expectations, on a comparison between rate at which the factor substitution changes the marginal product of capital and the rate at which the real capital income changes.

Model B

In this model, Hahn chooses to abandon the assumption of perfect wage adjustment. The real wages can go beyond the equilibrium value at any time with decreasing prices. That means that the factor substitution happens not only because of difference between the real capital income and the marginal product of capital but also, because of differences between the real wage rate and the marginal product of labour. When the prices fall, the distribution of income moves in favour of the workers. And that – if we take into account the Kaldorian assumptions – is supposed to raise the propensity to consume.

So with a raise of w such as $w>w^e$, we shall have :
- at every level of the production process, a factor substitution ;
- $Y<Y^e$
- the change in the income distribution might increase the consumption demand but it could also reduce the investment demand : $\downarrow Y \rightarrow Y < Y^e \rightarrow \frac{K}{L} \uparrow \rightarrow I \downarrow$

Main conclusions

Hahn conducts a thorough investigation of each model, giving us a description of the behaviour of each system when out-of-equilibrium. He concludes that the model A *"would have a stable full employment equilibrium with constant prices if (a) interest rate are constant but expectations are sufficiently inelastic or (b) if the demand for investment is relatively unresponsive to changes in the real interest rate or (c) if interest rate are moderately flexible."* For Hahn, model B allows us to have a closer look at the role of factor substitution in the stability of the equilibrium :

"Factor substitution is important to stability because it greatly increases the responsiveness of the system to the price mechanism. If there were no possibility of factor substitution, the chances of interest rate or wage changes stabilizing the system would be much more slender since producers would have to contend with the deadweight of excess capital. But when that has been granted, it remains true that the speed with which producers react to "errors" remains of the greatest importance to stability and since this "reaction speed" depends amongst other things on the degree

of confidence in the continuance of equilibrium, some rather old-fashioned propositions in business cycle theory regain some of their respectability."

Expectations and investment are at the centre of his argument : he is definitely on the path of a certain synthesis between long and short run growth equilibrium.

Hahn and Solow

In 1959, Robert Solow and Frank Hahn held a correspondence just before Hahn released his 1960 paper in the Quaterly Journal of Economics. The remarks made by Solow gives us an insight of his position towards the question of expectations and investment. It testifies that Solow had those issues in mind, even if those were absent of his current work (Assous, 2013).

"Of course you're right and Nicky's [Kaldor's] model simply will not stand up under scrutiny. When it is not self-contradictory it is more or less completely arbitrary. As you realize, however, there is one respect in which it is a step in the right direction. The Harrod- Domar legacy of paying attention only to equilibrium paths is by now an obstacle. All these ad hoc stability statements about what happens off such a path are useless without an explicit causal dynamics. But if we had the latter, then the equilibrium paths would appear as certain special-motion and one could deal with them directly." (Solow to Hahn, March 23, 1959)

4.2.3 Finally : An unsatisfied Robert Solow

An old concerned

So Solow, even if often considered as the champion of equilibrium model with his 1956 contribution, also had in mind the out-of-equilibrium type problems. An interesting hypothesis would be to think that Solow was actually in quest for that medium run growth equilibrium (Assous, 2013) and that his work reflects that quest.

As I mentionned earlier in this paper, Robert Solow distinguishes himself by being an economic theorist and an applied economist. His main concern is to be able to design model that would match empirical data. But in the beginning of the 1960's, he has obviously conscience that his 1956 model – despite its elegance - does not match those requirements and he is puzzled about it :

"In the shortest run, given capital means given concrete items and of course you have no objection to that. In the shortest run capital is like land, unalterable, fixed, etc. In a longer run, some of the concrete items are not fixed. But something is fixed : asset preferences, savings habits, relation among these things and the rate of profits, time preferences, the amount of accumulation already done, whatever, so in a longer run, given these things, and given the technological

input-output relations, the production function relating possible lists of physical inputs to possible lists of physical output, I deduce which among the possible forms of capital goods will be constructed in equilibrium. Now to get from one equilibrium to another, when a disturbance (like a change in the production function) occurs, I don't know. But how to compare equilibrium, and to find something which can be common - described as substitution between labor and capital, I do know." (Solow to Nicholas Kaldor, January 30, 1961)

In his 1964, Solow is the president of the Econometric Society – succeeding Malinvaud. In his presidential speech, he expresses himself as such :

"Its importance goes far beyond the desirability of being able to predict how output perman hour will change from quarter to the next. Art Okun or Ed Kuh or Dan Suits can already do that. What I am looking for is a way to unify the economics we teach our students and the economics we use when we advise governments and analyze passing economic events, and do it in a way amenable to econometric treatment. This patching up of theory to explain experimental uniformities is the way science usually proceeds." (Solow 1964 : 29-30, skipping inserted page 29a)

The outcome of those reflections are to be seen in the 1968 article, he co-writed with Joseph Stiglitz.

Solow and Stiglitz (1968)

Just as Sen's paper did in 1963, this paper is *"an attempt to clarify the relation between two alternative theories of distribution"* : marginal productivity theory and Post-Keynesian theory. Even if the model used is clearly a short-term type one, that article can be seen as a step toward medium-run growth model. As a matter of fact, they studied their model in terms of trajectories, investigating the impact of disturbances on stable or on unstable equilibrium. That article looks like a compilation of the questions and critics that were emphasized in the previous year. Stiglitz and Solow try to deal with those in a systematic way, taking into account even Kaldorian assumptions. In their concluding remarks, the authors underline the importance of the quest for a synthesis between long and short run macroeconomics, giving new leads for the future :

[...] the model needs to be extended to the long run. The first requirement is to find a representation of the shift in short-run production possibilities brought about by current investment. The easi-est course is to suppose that the short-run production function is simply a section of a long-run production function in capital and labor. If this is too great a stretch of the imagination, there are more plausible – but less manoeuvrable – alternatives ; see, for example, Solow, Tobin, Weizsacker, and Yaari and Attiyeh. Depending on how this task is accomplished, there may or may not arise the further question of the choice of labor-intensity for current investment. When

a choice of technique is available, the current and prospectivep rice configuration will have an influence on the labor-intensity selected for any given increase in capacity. Moreover, since the price configuration has a lot to do with the profitability of any given investment, it will have an influence on the amount of capacity installed. In the long-run context, investment cannot be treated as exogenous, even as an approximation.

Conclusion

In this paper, I tried to give an accurate view of two specific intellectual communities which worked on growth economics during the $1956 - 1968$ period. The main teaching that I drew out this work is the difficulty to clearly identify those groups. Inside and outside those communities, there were intricate relationships that influenced the process that lead to the idea of medium-run macroeconomics and made the economic science evolve. As a matter of fact, the opposition between the MIT researchers and those from Cambridge UK – often restricted to the Controversies on the Capital – covered a more profound field of inquiries. Our protagonists knew the limits of their models and tried to dig up some solutions through the analytical linking of their respective theory and vision of the world. I support that the exchanges that were held between the members of each communities of understanding testifies that they were aware of their limits and that were waiting for a better solution to fill the weaknesses of the growth theory : a working medium-run growth model. But as Solow expressed himself in his Nobel Prize speech :

"Growth theory was invented to provide a systematic way to talk about and to compare equilibrium paths for the economy. In that task, it succeeded reasonably well. In doing so, however, it failed to come to grips adequately with an equally important and interesting problem : the right way to deal with deviations from equilibrium growth." (Solow 1988 : 311)

Macroeconomic theory has not yet successfully taken up that challenge offered by growth theory but, undoubtedly, some future economists will take interest in the matter.

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